be found on comparing pp. 191 and 313. On p. 191 we have a short abstract of an article on petroleum, by A. Riche and G. Halphen. On p. 313 we have a long abstract of the same article. In one case it is given under *qualitative* organic chemistry, in the other under *quantitative* organic chemistry. Yet the reference in each case is the same—"*J. Pharm. Chim.*, 1894, 30, 289." In this case, therefore, the abstracts are not even prepared from different journals.

I would suggest, then, that the first reform which the Editing Committee might institute in carrying out their scheme of retrenchment, should be one placing a limit on the number of abstractors who are to deal with one and the same article, even when it occurs in different publications. JAMES HENDRICK.

Glasgow, October 2.

Note on the Dendrocolaptine Species, "Dendrexetastes capitoides" of Eyton.

It recently became necessary for me to examine some of the Dendrocolaptine birds in this museum, and among them the species named above. Our specimen, the type of the genus *Dendrexetastes* founded by Eyton in 1851, in Jardine's "Contributions to Ornithology," on a skin from an unknown locality, formerly in Lord Derby's museum, has evidently been examined by Dr. Sclater, for its label bears, in the well-known calligraphics of that distinguished authority on this group, the name *Dendrexetastes temmincki.* The difficulty I have in ascribing our specimen to that species is the cause of this note. According to the fifteenth volume of the "British Museum Catalogue of Birds," by Dr. P. L. Sclater, the genus contains but two species, *D. temmincki* and *D. devillii*, which, by his key on p. 140, are distinguished from each other, the former by having "blackish cross-bands" on the belly, and the latter having that region "uniform brown." On consulting Eyton's original description in the "Contributions to Ornithology," I can find no mention of any cross-bands on the belly; for there are none on the skin, which is apparently that of a mature bird. In looking up next the description by Lafresnaye, in the "Revue de Zoologie" for 1851, of his *D. temminckii*, to which Dr. Sclater has relegated as a synonym Eyton's *D. capitoides*, I read:—" . . . . pectoris ventrisque plumis totis umbrinis, in medio maculâ triangulari-elongata nivea nigro marginata notatis; ventris maculis strictis; fere linearibus; subcaudalibus pallide rufescentibus, albo late, fuscoque anguste vittatis. . . ." These words, as I interpret them, make no mention of the presence of cross-bands on the belly of *D. temmincki*, while the latter half of the quotation, in regard to the under-tail-coverts being pale rufous, with broad white and narrow fuscous spots, does not apply to *D. capitoides*, for the type-skin before me presents no such characters. The plate illustrating Lafresnaye's description of the first-mentioned bird (*loc. sup. cit.*) shows its breast-spots to be much narrower, though not linear, and shorter than those in *D. capitoides*, while the spots on the feathers on the upper part of the belly can hardly be termed "fere linearibus," which they are, however, in *D. capitoides*. The lower belly in the plate, "plumis totis umbrinis," shows, just as in the last-mentioned species, not a single cross-band. It would appear to me, therefore, that *D. capitoides*, Eyton, can scarcely be = *D. temmincki*, Lafr., while the latter differs from *D. devillii* (of which I regret our museum does not possess a specimen), and, I take it, from *D. capitoides*, by its smaller and narrower throat-spots. The subcaudal characters separate *D. capitoides* from *D. temmincki*, and apparently the typical *D. devillii* is separated from it also by the "striis strictissimis" of the breast, and the very linear shaft-stripes of the upper neck feathers. Is *D. capitoides* = *D. devillii?* Or are there three species? I incline to the opinion that there are. HENRY O. FORBES.

The Museums, Liverpool, October 8.

The Pressure of a Saturated Vapour as an Explicit Function of the Temperature.

It may be of some interest to note that Cailletet and Mathias' "Law of Diameters," in combination with any equation of state, such as Van der Waals', which applies to the region of coexistence of liquid and vapour, supplies an (empirical) expression for the maximum pressure of a vapour at any temperature T in the form of an explicit function of this temperature and known constants.

Let p, v and T denote the pressure volume and absolute temperature of unit mass of the substance. According to Van der Waals' original equation of state, we have then:—

$$\left(p + \frac{a}{v^2}\right)(v - b) = \mathrm{RT}.$$

If v_1, v_2, v_3 denote the roots of this cubic in v, we have:—

$$v_1 + v_2 + v_3 = b + \frac{\mathrm{RT}}{p} \; . \; . \; . \; . \; . \; . \; . \; (\text{i.})$$

$$v_1v_2 + v_2v_3 + v_3v_1 = \frac{a}{p} \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; (\text{ii.})$$

$$v_1v_2v_3 = \frac{ab}{p} \; . \; . \; . \; . \; . \; . \; . \; . \; . \; (\text{iii.})$$

Now, for any definite value of T less than the critical temperature, these equations give, when we put p equal to the maximum vapour-pressure corresponding to this temperature, three values, v_1, v_2, v_3, two of which (say v_1 and v_2) denote the volumes of unit mass of the substance in the states of saturated vapour and "saturated" liquid at this temperature. Accordingly, $\frac{1}{v_1}$ and $\frac{1}{v_2}$ denote the densities of the substance in these states, and the law of Cailletet and Mathias, above referred to, enunciates that the arithmetic mean of these densities can be very fairly represented as a linear function of the temperature. Therefore we can write:—

$$\frac{v_1 + v_2}{v_1v_2} = \phi\mathrm{T} \; . \; . \; . \; . \; . \; . \; . \; . \; (\text{iv.})$$

where ϕ denotes a linear function, whose two constants are known.

Eliminating v_1, v_2, and v_3 from the four equations (i.), (ii.), (iii.) and (iv.), we readily obtain:—

$$p = \frac{\mathrm{RT}(1 - b\phi\mathrm{T}) - a\phi\mathrm{T}\,(1 - b\phi\mathrm{T})^2}{b^2\phi\mathrm{T}} \; . \; . \; . \; . \; (\text{v.})$$

This result simply amounts to the following:—

If we fix the temperature T of a vapour, then the maximum vapour-pressure at this temperature is completely determined, *i.e.*

$$p = \mathrm{F(T)}.$$

Similarly the sum of the densities of saturated vapour and liquid in contact with it is determinate if T is fixed, and thus

$$\frac{1}{v_1} + \frac{1}{v_2} = \phi(\mathrm{T}).$$

Equation (v.) shows that the former function is known if the latter be known, and as Cailletet and Mathias have shown that the latter is very approximately linear, we can give the form of F(T).

This result, however, is not of any practical use unless the equation of state does really apply with good approximation to the region of liquid and vapour. F. G. DONNAN.

Holywood, Co. Down.

Colours of Mother-of-Pearl.

In numerous text-books the colours of mother-of-pearl are included amongst phenomena of colour produced by striated surfaces, and though it is conceded that only a part of the colour is due to this cause, that part is generally assumed to be, at any rate, an appreciable quantity. Experiment will show, however, that such is not the case. When the colour produced by the striations is viewed in an impression of the pearl on sealing-wax or gelatine it is visible, though it is totally different in character from the iridescence of the pearl itself, in which the tiny contribution of colour from the striations is completely overpowered by that due to another cause. In white mother-of-pearl the striations are often as close together as in coloured varieties, and at certain angles, when viewed by light from a defined source, there is a little colour visible in the white specimens; just so much, and no more, is contributed by the striations of the coloured specimens, as may be shown by viewing a piece under the surface of water, when the effect of the striations is necessarily abolished, though the iridescence is not at all appreciably diminished. The whiteness of some varieties must be attributed to a different thickness or greater opacity of the laminæ. It is

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these laminæ which, acting as "films," give rise to all the colour of nacre, practically; and the phenomenon should be included amongst those of colours from "films," and not from "striated surfaces," the latter being merely incidental, and for all practical purposes contributing nothing to the effect.

C. E. Benham.

A RATIONAL CURE FOR SNAKE-BITE.

When it was established beyond dispute or cavil that the serum obtained from animals, immunised against bacterial infections and intoxications, possesses in a marked degree antitoxic powers—as distinguished from antibiotic powers—and that such serum when mixed in a test-tube with the bacterial poison in question will, so to speak, neutralise the toxic effects of such poison, however deadly, it was merely a question of time, opportunity, and patience that attempts would be made to extend the principle of serum-immunisation to other, *i.e.* non-bacterial, poisons. Ehrlich was the first to show us the way. He gradually accustomed animals to withstand comparatively large doses of abrine, ricine, and robine, three vegetable toxines, all belonging to the group of proteines, reacting as albumoses or globulines. In that manner he produced in the animals a relative immunity, or perhaps, more correctly, a tolerance. He found that though subcutaneous inoculations lead to better results, that this immunity can be brought about also by feeding. In whatever way the animal is prepared, its serum eventually acquires specific antitoxic, immunising, and curative properties. It was thus demonstrated that the wonderful discovery of Behring and Kitasato—for which Behring, however, claims the sole credit—has a scope much wider than at first was dreamt of. Behring himself, to begin with, explained the action of the serum as antibiotic or germicidal; but it soon became evident that, though when injected into the animal body it causes the destruction and death of the infective pathogenic organisms, nevertheless its chief action is "vitally" antitoxic. For working with the tetanus toxine alone, separated from the bacilli which produced it, its deadly effects can be readily neutralised by a few cubic centimetres of a powerful serum. And if we remember that ·23 milligram of tetano-toxine would represent the fatal dose for a human being weighing 70 kilogrammes, then we can get an idea as to what extraordinary changes must have been produced in the serum, or rather in the blood and tissues, of the immunised animal, to enable its serum instantaneously to remove the lethal effect of the toxine. The only poison comparable to tetano-toxine in virulence and rapidity of action is cobra poison, and it also resembles chemically the bacterial toxines, reacting as an albumose, though for the sake of accuracy it must be mentioned, that the poison of tetanus has been clearly shown by Brieger, Cohn, and Sidney Martin not to be an albuminous body, and that possibly most of the bacterial toxines may turn out not to be albuminous substances. Still, so far as our present knowledge reaches, cobra poison and other snake venoms are chemically closely allied and analogous to the "toxalbumins" of bacteria.

It had also been demonstrated by several observers,[1] that by means of oft-repeated injections of small sublethal doses of snake poison (rattlesnake, cobra, or viper venom) the resistance of an animal against the poison may gradually be increased considerably, it may be rendered "giftfest," to borrow a German expression. In fact, all the methods used for inducing a tolerance against tetanus poison can be shown to work in the case of cobra poison (this is the poison generally employed). Thus Calmette, whose work in this line follows directly that of Sewall's and of the writer of this article, has shown that a so-called immunity can also be produced by gradually increasing injections of poison attenuated by heat, iodine, trichloride of iodine, hypochloride of calcium, &c.; in fact, the analogy is complete. From this stage, at which others had already arrived, Calmette went ahead with Phisalix and Bertrand. Having previously attempted both to prevent and to cure the effects of inoculation with cobra poison by means of chloride of gold—wherein, however, as shown by the writer,[1] he failed—he directed his attention at once to the serum of immunised animals, and in February 1894 he showed, before the Société de Biologie, that on mixing cobra or viper venom with small quantities of serum obtained from an immunised rabbit the deadly effect of the venom disappears, a fact at once confirmed by independent observations of Phisalix and Bertrand. In May 1894 and in April 1895, Calmette published two concise papers in Pasteur's *Annales*, containing a full account of his results. These, briefly summarised, are as follows: (1) The serum of an animal immunised against snake poison (he used poisons of the following snakes: *Naja tripudians and haje*, *Crotalus durissus*, *Bothrops lanceolatus*, *Cerastes*, *Pseudechis porphyriacus*, *Hoplocephalus curtis and variegatus*, *Acanthopis antarctica*, *Trimeresurus viridis*) possesses properties similar to those which the serum of animals immunised against tetanus and diphtheria possesses. (2) The serum of a rabbit immunised against cobra or viper venom acts equally well against any of the other poisons, *i.e.* there is no specificity of action, as judged by the species of snake. (3) The serum possesses not only neutralising properties when mixed with the venom in a test-tube, but possesses also marked immunising and curative properties, *i.e.* poison injected after previous serum administration becomes powerless, and serum injected after previous poison administration neutralises the effects of the poison in the animal body, even after the symptoms of intoxication have already set in. Naturally the effect depends on the degree of immunity of the serum giver and on the proportionate amount of serum used. (4) The immunising effect produced by serum injections is not so lasting as that produced by direct injections of the poison, *i.e.* serum injections are incapable of rendering animals "giftfest." Calmette alludes to other matters, but since these are of secondary importance and still debatable, and not directly related to the subject of this article, we must pass them over. There is, however, one point which must be mentioned, since it is one affecting the whole principle of serum immunisation. He states that he has succeeded in producing a "Giftfestigkeit" by means of repeated intravenous injections of hypochloride of calcium, and that the serum of such "chlorinated" animals will neutralise, in the test-tube at least, the effects of cobra poison. Roux elsewhere mentions[2] that the serum of animals immunised against tetanus or rabies is capable of neutralising snake venom and of protecting other animals against subsequent intoxication with cobra poison, and that rabbits vaccinated against rabies can withstand four to five times the lethal dose of cobra venom; and also that abrine serum will counteract the effects of cobra poison, and cobra serum those of abrine. Calmette goes so far as to say that an animal vaccinated against abrine may acquire a relative immunity against diphtheria, ricine, and anthrax. If this be so, we shall have to modify our views as to the specific action of antitoxic serum, *i.e.* the first principle of serum therapeutics. We require a number of control observations before we can accept these remarkable statements; partial contradiction they have already received from Germany,[3] and the

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[1] Sewall, *Journal of Physiology*; Kanthack, *ibid.*, 1892, vol. xvi. Nos. 3 and 4; Phisalix et Bertrand, *Compt. rend. de l'Acad. d. sc.* cxviii. 1894, pp. 288, 356; *Arch. de physiol.* 5 sér. vi. 3, 1894, pp. 567, 611; *Compt. rend. de la soc. d. biol.* 9 sér. vi., 1894, pp. 111, 124; Kaufmann, *ibid.*, p. 113, and Calmette, *Annales de l'Inst. Past.* 1894, vol. viii. No. 5, p. 281.

[1] *Lancet*, June 11, 1892. The uselessness of strychnine was previously demonstrated by the writer in his paper in the *Journal of Physiology*.

[2] *Annales de l'Inst. Past.* 1894, No. 10, p. 722.

[3] Ehrlich emphatically denies any such vicarious counteraction with regard to abrine and ricine (cf. *Deutsche Med. Wochenschrift*, vol. xvii. No. 44, p. 1218).

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